Vol. 20, No. 10 PSYCHOLOGICAL REVIEW PUBLICATIONS October, 1923

Psychological Bulletin

CONTENTS

PUBLISHED MONTHLY BY THE

PSYCHOLOGICAL REVIEW COMPANY
37-29 COLUMBIA STREET, ALBANY, N. Y.
AND PRINCETON, N. J.

Agents: G. E. STECHERT & CO., London (2 Star Yard, Carey St., W.C.); Paris (16, rue de Condé)

Entered as second-class matter at the post-office at Albany, N. Y., September 25, 1922

Psychological Review Publications

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PSYCHOLOGICAL REVIEW COMPANY

ALBANY, N. Y., AND PRINCETON, N. J.

FOREIGN AGENTS: G. E. STECHERT & CO., London (2 Star Yard, Carey St., W.C.) Paris (16, rue de Condé)

Vol. 20, No. 10 October, 1923

THE
PSYCHOLOGICAL BULLETIN

INSTRUCTION IN PSYCHOLOGY IN PARIS[1]

BY MORRIS S. VITELES
University of Pennsylvania

The purpose of this report is to describe the opportunities for the study of psychology to be found in Paris. The courses given during the past academic year, the faculty, the laboratories and their equipment, the clinics, etc., are discussed, and a few general comments on French education and the organization of the instruction in psychology are made with the purpose of acquainting teachers of psychology in America and prospective students of this subject with the opportunities for the study of the science in Paris.[2]

The work in psychology in Paris is given under the auspices of the Institut de Psychologie, an organization[3] "which has for its object instruction in the theory of psychology and in applied psychology and research within the realm of the psychological sciences. It unites and groups in a common center opportunities for study which already exist, organizes new courses, and establishes close coöperation among all. It offers each year to French students and to foreign students a cycle of complete and coherent psychological studies reinforced by

[1] Extracts from the report of the author, as American Field Service Fellow in Psychology at the University of Paris.

[2] This paper is limited to a discussion of the opportunities for study in Paris because the author made intimate contact with French instruction in psychology in Paris only. It is his understanding, however, that the major work in psychology is centralized in Paris, and he has been led to believe that the description of opportunities for the study of this science in Paris is equivalent, from the point of view of the foreign student contemplating study in France, to a description of the opportunities in France.

[3] *Livre de l'etudiant*, Université de Paris, 1922–23, pp. 175–176.

practical work. It organizes and sustains by all means in its power original research on the part of professors and students.

The Institut de Psychologie is attached to the University of Paris, Faculties of Letters and Science, and placed under the scientific direction of the University of Paris of the College of France and the "Écoles des Hautes Études." There is no separate and distinct department of psychology at the University of Paris. However, in the way of practical administration, the Institut actually carries out the majority of the functions ordinarily assigned to the departments of psychology in American universities. It receives an annual budget from the University for the purchase of material and for administrative expenses entailed in organizing courses of study and experimental research. In one very important respect it differs from a department. The Institut has been granted neither the authority nor the financial means to attach to itself professors and instructors directly responsible to the Institut. Professors attached to the various faculties of the University of Paris and to the College de France voluntarily undertake to give courses under the auspices of the Institut, but they receive no payment from the Institut and the content matter of their courses and their organization are not subject to its supervision.[4] The Institut functions only in coördinating, in so far as it is possible under such a system, the courses given by men attached to different schools and in registering, examining and certifying students who follow these courses. The Institut is the center of psychological research, the rallying point from which it is hoped will grow a department imbued with the power and the recognition accorded to other departments of study in the University of Paris. However, it must be recognized that a curriculum organized by such an Institut does not achieve the unity, coherence and the inter-relationship among the individual elements of a curriculum organized by a department of psychology such as found in American universities, which has the power of selecting the courses to be given from year to year, of selecting the men to give them, and of administering each element of the curriculum with the purpose of systematically training students in psychology.

"Research work[5] leading to diplomas for advanced studies to

[4] The Institut is empowered to pay "indemnités," that is, fees for individual lectures and, in some cases, lecturers not attached to the University of Paris or to the College de France are paid for series of lectures in this way.

[5] *Livre de l'etudiant*, Université de Paris, 1922–23, p. 178.

the doctorates can be followed in the laboratories of the Institut. In addition, the Institut de Psychologie gives certificates (in psychology, pedagogy and applied psychology) to students registered for two semesters who have followed regularly the courses in the particular section and who pass examinations at the end of each semester."

During the past academic year, and also in other years, the following offered instruction under the auspices of the Institut de Psychologie. Occasionally others are added to this list.[6] Pierre Janet, Professor of Experimental Psychology, College de France; George Dumas, Professor of Experimental and Pathological Psychology, Faculté des Lettres, Université de Paris; Delacroix, Professor of Psychology, Faculté des Lettres, Université de Paris; Piéron, Professor of Psychology of Sensations, College de France, Director of the Laboratory of Physiological Psychology of the Sorbonne; Wallon, Doctor of Medicine, Agregé en Philosophie (not attached to a faculty); Simon, Director of the Institute for Feebleminded, Perray, Vaucluse; Meyerson, Assistant Director of the Laboratory Asile Clinique; Rabaud, Professor of Experimental Biology, Faculté des Sciences, Université de Paris; Fauconnet, Maître de Conferences, Faculté des Lettres, Université de Paris.

Others should be mentioned whose work, although not conducted under the auspices of the Institut de Psychologie, will be of interest to the student of psychology in Paris. Among the medical men whose work in mental pathology is of particular interest may be mentioned Claude (Clinique de Faculté de Medicine), Toulouse (who conducts a Service de Prophylaxie Mentale at the Asile Clinique) and Colin. At the Asiles de la Seine interesting work is carried on by a number of others, among them Simon and Mignard.

Among the philosophers the lectures of Levy-Bruhl (Membre de l'Institut, Professor of Modern Philosophy, Faculté des Lettres, Université de Paris), who has done considerable work in the field of social psychology, of Brunscchvigg (Membre de l'Institut, Professor of General Philosophy, Faculté des Lettres, Université de Paris), and of Basch (Professor of Aesthetics and the Science of

[6] In addition to these listed Professor W. B. Pillsbury, of the University of Michigan, gave during the second semester of the current academic year two series of lectures at the Sorbonne under the auspices of the Institut de Psychologie. In one series he discussed "The Development of Psychology in America" and the second series he devoted to a general discussion of the rôle of psychology in political and social progress.

Art, Faculté des Lettres, Université de Paris) are of special interest to the psychologist.

Among the sociologists and educators, the work of Bougle, of Fauconnet, of Mauss, of Hubert, and of Granet borders occasionally upon the psychological. Others who work in allied fields should, perhaps, be mentioned: Mayer (Natural History); Meillet (Linguistics); Lapicque (General Physiology); Bohn (Biology and Psychology); Seignobos (History). They have an interest in psychology and are also interested in establishing relationships between the facts of their fields and those of the science of psychology.

In addition to the courses mentioned above there are courses in anthropology given by Manouvrier, Mortillet, Anthony, Paul-Boncour, and others, which may be followed with profit by the student of psychology. During the current year, for example, there were to be found courses on "Les caracteres sexuels secondaires dans l'espece humaine" (Manouvrier); "Les stimuli psycho-sociaux" (Papillault); "Les conditions psychiques de reactions anti-sociales" (Paul-Boncour), given under the auspices of the École d'Anthropologie and the Institut International d'Anthropologie.

As in the American universities the work in the field of psychology given under the auspices of the Institut is divided into: (a) Lecture Courses, of two kinds: (1) Cours Publiques, open to all students enrolled in any department of the Sorbonne and to the general public, and (2) Cours fermés, which may be attended only by students enrolled in the Institut de Psychologie; (b) Conferences, somewhat similar in character to the American seminary, in which professors meet a small group of students (meeting certain qualifications established for each course) to discuss in greater detail the subject matter taken up in the lecture course, to direct, criticize and discuss the preparation of papers by the students; (c) Experimental Work in laboratory and clinic, in which the student carries on experiments under the close supervision of the laboratory instructor, and some opportunity for original research; and (d) Observations in and Visits to Institutions. In the field of abnormal psychology Paris is replete with opportunity for the observation of the examination and treatment of cases of mental and nervous diseases. There are not only formal courses involving demonstration of neuropsychiatric cases, but practically all the clinics can be visited with a minimum of formality, so that the foreign student can readily follow the work of the most noted French specialists in the field which interests him most. In addition to courses in the field of mental and nervous

diseases of adults there are a few courses which concern themselves with the observation of the mental disorders of children, and one course in which visits are made to institutions for the training of normal and abnormal children and to schools applying special systems of training, such as the Montessori method, the Braille method for blind children, etc.

There is no systematic organization of courses by which the student passes from one to another, from a general course to an intensive study of one or another body of psychological fact. There is no arrangement by which the student passes, for example, from the intensive study of simple mental processes to the intensive study of more complex mental processes, by which he proceeds from the consideration of one body of psychological facts to the consideration of another having its basis in the first. Every student is eligible for attendance in all courses, at all times. He takes them in any order, following each year any course, which may or may not bear a relationship to another in a preceding year. In general there is a relationship among the courses given by the same lecturer in different years, but among the courses given by different lecturers there is not necessarily or generally such a relationship. The function of organizing the great variety of material with which the student comes into contact under such a system is altogether a function assigned to the student and not encouraged by the method of instruction at the Sorbonne.

This liberty in education is not limited to the work in psychology, but is characteristic of French university education in general, and particularly of that in the Faculté des Lettres and Faculté des Sciences. Here the old doctrine of extreme liberty which permits the student to attend classes when he pleases and to dispose of his time largely as he pleases still prevails. It is only recently, according to M. Meyerson, that even in the Medical School have compulsory courses and compulsory attendance at classes won over the old system under which the student attended such courses as he pleased, when he pleased, and was checked up only in the yearly or semi-yearly examinations.

To some extent, partly as a result of American influence, there is a growing tendency in other faculties to organize more carefully the courses in a particular field and to make attendance upon certain courses compulsory. The Institut is, in a sense, an attempt at such organization but its work has only begun.

The opportunities for experimental work in the field of general

psychology are very limited. There are two laboratories in which experimental work can be done.

The Laboratory of Physiological Psychology of the Sorbonne is under the direction of M. Piéron and M. Meyerson. The space is limited to two small rooms and a small cabinet which is used as a dark room. The Institut is in possession of an assortment of the apparatus which is needed in a well-equipped psychological laboratory, but much of it is not used by reason of the limitations of space. The Institut is also in a position to purchase additional apparatus, but it finds it very difficult to procure from the University authorities the rooms needed to house both the apparatus which it already possesses and such new equipment as it may purchase.

For the advanced student opportunities for experimentation are limited. The character of the equipment and the interest of the director of the laboratory, M. Piéron, make the fields of sensation and perception the most productive fields of experimental investigation in the field of general psychology in Paris. This is particularly true for visual sensation and perception. In spite of the limitations for experimentation in other fields the stimulating influence of M. Piéron, who has himself done some very valuable and original work on perception, and who is well acquainted with the work of others, makes Paris a desirable place of study for one interested in these fundamental problems.

The Laboratory of the Asile Clinique is a fairly well equipped laboratory of experimental psychology, under the direction of Dr. Toulouse, assisted by M. Lahy, at the Asile Clinique of the Hospital St. Anne. The laboratory includes one large room, two slightly smaller rooms, a workshop for the repair of apparatus, and a small library. One of the rooms can be transformed into a dark room. The laboratory is associated with a psychiatric hospital and the equipment is naturally of a kind which can be used in obtaining experimental data on mental disorders. There is, moreover, a very lively and growing interest in this laboratory in vocational psychology. Equipment which can be used in experimental investigations in the latter field is being made and purchased.

During the past academic year lecture courses in general psychology were given by Janet (L'Evolution de la memoire et la notion du temps), by Delacroix (Langage et Pensée), and by Piéron (Les fonctions perceptives). Of these only the course of Piéron could be considered purely psychological in character. An elementary

laboratory course in experimental psychology was also given by Piéron.

Lecture courses on animal behavior have been given during recent years by M. Rabaud, of the Faculté des Sciences rather from the standpoint of zoölogy than that of psychology. Dr. Rabaud has been recently authorized to organize and equip a laboratory in which he intends to encourage research in the field of animal behavior.

In abnormal and clinical psychology lecture courses have been given by Dumas, by Meyerson, and by Wallon, supplemented by observations and demonstrations of cases in Hospital St. Anne, and in a school for boys.

There is a growing interest in Paris in the problem of educational psychology. There is a special Section de Pedagogie of the Institut de Psychologie and a number of courses in this field have been given during the current year, by MM. Simon, Wallon, and Fauconnet. The course by Fauconnet was devoted to an historical review of educational thought and methods. Experimental work in the field of educational psychology, with the exception of the work with anthropometric tests and the Binet-Simon tests conducted on a small scale by Simon in the laboratory of Alfred Binet is practically nonexistent. There has been a little work accomplished in the provinces and some has been done in the schools of Paris, but there has been no large scale attempt to evaluate methods of education, to investigate the usefulness of mental and educational measures in education such has been made in America and in other European countries. The French educational system is so organized that it is difficult for the psychologist to gain entry to the schools for the purpose of experimentation. The centralization of authority, the regulations of curriculum by governmental decree, the difficulty of breaking into a daily program without governmental authorization, are facts to be considered in this connection.

There has been no large scale experimentation in France with tests for measuring the general competency and specific abilities of children and adults. The movement toward mental testing which Binet started has rested almost at the point at which he left it. There has been no revision of the tests which were developed by him in coöperation with Simon. A lecture course on "La Mesure en psychologie; les methodes de tests; le calcule de correlations, etc.," was given during the past year by M. Piéron. There was no actual demonstration of tests, but the problem of testing and tests in use in other countries were academically discussed. M. Simon also gave

a laboratory course on the use of anthropometric and the Binet-Simon tests.

There is a growing interest in vocational psychology throughout France. Actual experimentation in the fields of vocational guidance and industrial psychology has been carried on by M. Lahy. He has worked mainly with typists and motormen, but he is gradually extending his work into other fields. In the laboratory and in a number of industrial establishments with which he has established connections there is opportunity for individual work in industrial psychology for the advanced student. Only a student with a thorough command of the language can carry on such work successfully. A background of thorough training in the elements of industrial psychology and preparatory experiments in American industrial plants should also be possessed by the student who wishes to undertake original research in this field in France. The only course in this field was one by M. Lahy on "Techniques de psychologie appliquées à la determination des aptitudes," an elementary course covering a number of the tests of special aptitudes such as used in early American investigations.

The work in psychology in Paris is better adapted for the student who is well along in psychology than for one who is just starting graduate work in this field. The advanced graduate student limiting himself, as he usually does, to one particular bit of research in a given field, can, in most cases, more easily find the opportunity to satisfy his interest than the younger graduate student who, of necessity, scatters his interest over a number of fields of experimental investigation. He is, moreover, able to profit more readily from the comparison of French institutions, which he is in a position to visit, with similar institutions in his country. But, even if the advanced graduate student finds no opportunity for individual research within his field, he is in a position to profit from the very excellent critique of current theories and practices in psychology (general, comparative, educational, etc.) presented by the proverbially critical, rational Frenchmen and from the newer concepts occasionally suggested by such men as Janet, Dumas and Piéron.

THE EIGHTH CONGRESS FOR PSYCHOLOGY

ARRANGED BY THE SOCIETY FOR EXPERIMENTAL PSYCHOLOGY, LEIPZIG, APRIL 17–20, 1923

BY W. ELIASBERG

Munich

At the present time the Society consists of 225 ordinary members, but 900 persons participated in the Congress, among them guests from England, Switzerland, Holland and Japan.

The central discussions were devoted to investigations of personality. Felix Krüger (Leipzig) introduced the subject with a report on the notion of structure in psychology. The problem of personality, as well as that of individuality which is dependent upon it, forms a connecting link between psychology and philosophy. Both disciplines require the notion of structure. With this fundamental idea Dilthey started in his philosophy of culture of the present time, and furthermore he anticipated important results of later psychology. Coherence of structure signifies first of all that immediately experienced totality, to which in complex events all parts are more or less intimately, but never summarily, thrown together. Because of von Ehrenfels we know more of this fact of totality in all parts of the event. It realizes itself as sentiment in the actual total content of consciousness, not being at all dependent upon the simultaneousness of the parts in the course of high emotion or will-action. We dispose of special notions in order to denote the common element, the quality of complexes and of general totality. Sentiments and shaped occurrences are special kinds of complex qualities. On the other hand, the expression structure ought to be reserved for dispositional facts, especially for dispositions to mass occurrences, and for the sum total of such dispositions, viz., personality. The peculiar structure of our inner experiences is immediately represented by the dimensions of sentiments. Individual structures as defined above are accessible to experimental determination and measurement to a narrowly circumscribed extent, as compare for examples the newer researches on

consciousness, *Einstellungen, Aufgabebewusstsein* (Külpe and his school); *determinierende Tendenzen, Begabungen,* and *Korrelationen.*

By no means, as Dilthey believed, is the correlation of means and purpose essential to all psychological structures. The idea of purpose, in the abstract conception of William Stern (*Die menschliche Persönlichkeit,* 2 Aufl., 1920) is not suited to conceive these form-precisions of psycho-physical life. He who wants to comprehend culture scientifically must bring together psychological research and purely historical. It was the great accomplishment of William Dilthey to have done this. Overindividual sense and legal values disclose themselves only to a normative philosophy, which in the end must be proved metaphysically. But all interpretation of culture pretending to be true presumes psychological analysis, especially genetico-psychological. The idea of evolution which in Dilthey's most productive period comprised all discussions of spiritual structures was afterwards neglected by his pupils. Therefore, historically conditioned complexes of cultural things, like nationality and the mother tongue, are not adequately treated by them.

A second report was given by Otto Selz (Bonn) on types of personality and the methods of their definition. He pointed out that the investigation of the types of personality is being continually advanced to the foreground in the historical sciences, in psychology, and in psychiatry. For historical science the determination of the types of personality signifies an important new means of historical conception. For investigation of coherences in history, which are psychically conditioned, the historian makes almost exclusive use of the method of combining objective-spiritual events, utterances and actions and lasting products of the groups of individuals and communities. This method is an art, important as far as it is employed in a creative way, which history has along with other scientific methods. Two sides of personality have proved accessible to psychohistorical understanding: (1) the state of the world, as shown by deeds and personalities, and, (2) the directions of value, the tendencies leading instinctively or purposely to the realization of experienced values. This combination is particularly followed in Dilthey's inquiries. Eduard Spranger (in *Lebensformen,* 2 Aufl., 1921) has expressed his principal interest in the coherence, fixed by laws of structure, of the *Wertrichtungen* amongst each other. He applies the constructive method as exhibited in the deductive sciences. One of the admissible *Wertrichtungen* is thought to be a dominant one. This is examined to determine which variations in the others must result

from it. The predominant *Wertrichtung* proves itself everywhere as the central factor, which eventually decides the whole of human conduct.

Willy Peters (Mannheim) reported on inheritance, in an excellent paper which brought together in a fundamental way numerous works in the sphere of observation and experimental biology, of human endowments, and of psychopathology. Psychology may expect from this combination of investigations the clearing up of her idea of disposition.

On the basis of autobiographical-lexigraphical statements in the compilations *Who is it?* and *Who's Who* investigations were made by Giese (Halle) on the compensatory value of personality, to discover which sphere of activity plays a part next to the principal vocation. The questions here are: Which are essential vocations, to live upon (*Nährberufe*), and which are activities for rest, or favorite avocations or recreations? Activities in administration, in jurisprudence, and especially in pedagogical science have a universal compensatory tendency, and a similar statement may be made regarding organization work, and publicity and commercial activities. Technologists and sculptors have slight compensatory tendencies. The male sex shows this tendency greater than does the female, and North Americans in general have a remarkedly stronger compensatory tendency than Germans. Statistical examination of the records show such marked correlations that the functional coherence cannot be doubted.

Experimental determinations of differences in character and mental traits of animals were reported by David Katz (Rostock). With suitable tests, partly such as Katz and Révész formerly used with fowl, experiments were made regarding memory, conception of relation, counting, reaction to experiences in the labyrinth, as well as observations on spontaneous behavior. Animals capable of extraordinary performance showed imitation. In connection with the "sociological" problems of the poultry yard the work of Katz and Schjelderup should be consulted (*Zsch. f. Psychol.*, Vol. 88).

The paper by Gruhle (Heidelberg) on "Autobiography and Investigations of Personality" has already been published in enlarged form.

People living in a state of nature have been examined by Beck from Krüger's Leipzig Institution for Psychological Development. The problem was to determine the principal relations between the individual and his social surroundings, especially the state of indi-

vidualization and the process through which it is individualized. It was shown that the strongest collective-physical unions cannot prevent the I-myself of the individual from accenting himself, that he individualizes himself for instance in self-consciousness, pride in efficiency, etc. With primitive people such individualization is common. We thus must think of individualities and of emotional proceedings of individualization, while Levy-Brühl mostly thought of intellectual incidents and incidents of division of labor in order to explain the individual differentiation. Independent of objective social differentiations we see individualization tendencies grounded in emotional conditions.

A paper on the diagnosis of individuality (*Psychographical Teifenanalyse*) was read by Römer. Rupp used a test in which a visual model or a formless picture was to be completed somewhat after the style of expressionist performances. He showed that the task involves an optic-racial gift, which is not necessarily allied to intelligence, and Römer believes that it is possible to get from this one simple test an understanding of the world of feeling and thought and of the intellectual performance of the subject.

The psychology of children and nations was discussed from the point of view which E. R. Jaensch (Marburg) has designated "Eidetic" disposition. The fundamental peculiarity of youthful conception is the coherence between the world of imagination and perception, and in general the combination of experienced outer and inner worlds. Among students there appears occasionally a continuation of the eidetic type of youth, which approaches the general condition of many artists and philosophers of artistic disposition. The eidetic lower rank may be analyzed by experiment and by a similar proceeding we may have an enriched and deepened analysis of the upper rank. This was shown by Jaensch in his paper on differential psychology of nations. He found an eidetic disposition to a much higher degree among young French students than among Germans of the same age. The images of perception and imagination of the French youths and even their observations show a high plasticity, which means a great changeableness through other mental functions. This fundamental peculiarity of psychophysical constitution may explain other differences. Thus, P. Janet in his studies of neuroses in French patients thought the *fonctions du réel* to be the highest performances of man, because in the consciousness of the eidetical type reality of perception and imagination does not appear to be fixed and unalterable. The very loose contact with reality is essentially

incidental to the French nature. We find as a defense against the overgreat plasticity those curious inclinations to rule life according to fixed principles. These are prominent in the pathological-psychological examinations of Janet as well as in French memoirs and novels. That eidetical dispositions may be influenced is of the greatest importance from the educational standpoint. Freiling has demonstrated this by his investigations of the influences of instruction on the student type. He found that in classes in which the principle of occupational schools had been carried out, where the student was able to take an active part and develop himself, the percentage of strongly marked eidetics was unusually high. Thus the type of perception may also be influenced pedagogically.

In continuation of his work on the phenomena of religious events, Girgensohn (Leipzig) discussed the phenomena of religious thought. The method of investigation was that of the school of Külpe, especially that of Bühler, which requires self-observation of the exciting expressions of religious content produced by experiment. He contended that there exists no pure religious thinking, but the religious thoughts appear either as conditional feelings, for instance of devotion, or as figurative proceedings of fancy, or as processes of feeling. Nevertheless, in the religious process of thinking there are forms of thought the sense of which is independent of the form of occurrence and of the expressed statements. In the formation of the religious thought we find a wealth of individual manifoldness. In the logical form it is detached from the experiencing individuality.

Among the special studies which were reported mention should be made of the following: G. E. Müller, on the theory of arbitrary movement; Spearman, on the establishment of laws in psychology; W. Wirth, on the notion of consciousness; Blumenfeld, on the consciousness of work; Klemm, on the psychology of work; Volkelt, on drawings of children; Marbe, on insurance for accidents, and employment psychology; Kirschmann, on metallic luster; and Ettlinger, on the use of tools by animals.

THE SEVENTH INTERNATIONAL CONGRESS OF PSYCHOLOGY

BY L. L. THURSTONE
Washington, D. C.

The Seventh International Congress of Psychology was held at Oxford in the week of July 26–August 3. It was attended by about three hundred psychologists representing eighteen nations. The meetings were held in the University Museum. Rooms for the representatives at the Congress were provided by three of the Oxford colleges, namely, Balliol, New, and Manchester.

The officers of the Congress were as follows: President, C. S. Myers; Secretary, W. Brown; Assistant Secretary, W. J. H. Sprott; Local Secretary, H. Sturt.

The sessions opened with a business meeting on July 26, which was followed by a reception in New College by the vice-chancellor. The program of papers was as follows:

FRIDAY, JULY 27

9:30 A.M.—Symposium on "The Nature of General Intelligence and Ability," by Drs. G. H. Thomson, E. Claparède and L. L. Thurstone.

12:00 NOON—Dr. W. Koehler—The Problem of Form in Perception.

2:15 P.M.—Dr. G. Révész—Experiments on the Spatial Perception of Animals.

Dr. E. G. Boring—The Relation of the Limen of Dual Impression to Head's Theory of Cutaneous Sensibility.

Dr. Dwelshauvers—The Objective Registration of Mental Imagery.

8:30 P.M.—Prof. T. H. Pear, Mr. R. H. Thouless and Miss Ikin—The Psycho-Galvanic Reflex in Dream Analysis.

SATURDAY, JULY 28

9:30 A.M.—Symposium on "Does Progress in Educational and Social Science Depend on Progress in Psychology?" by Drs. M. W. Keatinge and P. B. Ballard.

11:15 A.M.—Dr. H. Piéron—The Psycho-Physiological Problems of the Perception of Time.
Miss M. Sturt—The Judgment of Time in Sleep.
8:30 P.M.—Dr. William Brown—Mental Conflict.

SUNDAY, JULY 29

8:30 P.M.—Mr. R. H. Thouless—The Psychology of the Contemplative Life.
Rev. Canon Streeter—Is Religion a Psychoneurosis?

MONDAY, JULY 30

9:30 A.M.—Symposium on "The Conception of Mental and Nervous Energy," by Drs. E. D. Adrian, Henry Head and C. S. Myers.
12:00 NOON—Dr. H. Sjöbring—The General Forms of Mental Activity.
2:15 P.M.—Dr. E. Mira—The Cardiovascular Changes in Mental Work.
Dr. Pierre Janet—Psychic Asthenia and Atony.
8:30 P.M.—Dr. Morton Prince—Awareness, Consciousness, Coconsciousness and Animal Intelligence from the Point of View of the Data of Abnormal Psychology.

TUESDAY, JULY 31

9:30 A.M.—Symposium on "The Classification of the Instincts" by Drs. J. Drever and Ernest Jones.
11:30 A.M.—Dr. J. F. MacCurdy—Instincts and Images.
Dr. S. Alrutz—The Psychological Importance of Hypnotism.
2:15 P.M.—Dr. Karl Abraham—Psychoanalytic Views on Some Characters of Early Infantile Thinking.
Dr. Alfred Adler—Advances in Individual Psychology.
8:30 P.M.—Mr. F. C. Bartlett—Symbolism in Folk Lore.

WEDNESDAY, AUGUST 1

9:30 A.M.—Symposium on "The Principles of Vocational Guidance," by Drs. O. Lipmann, Cyril Burt and L. L. Thurstone.
Prof. W. Moede—The Present Position of the Vocational Test in Germany.
Dr. G. van Wayenburg—Vocational Testing.
2:30 P. M.—Drs. C. H. Griffith and W. B. Pillsbury—An Experiment on Indirect Measures of Fatigue.
Prof. K. Koffka—New Experiments in the Perception of Movement.

Mr. H. Binns and Dr. H. S. Raper—A Comparison of Visual and Tactile Judgment in Individuals of Different Ages and Training.

Most of the papers were followed by open discussion, in which many interesting points were brought out.

Besides the formal papers there were informal meetings of smaller groups who had discovered common interests. These smaller meetings consisted generally in a continuation of the discussion which was started by the formal papers on the program. Among the topics which proved to be popular for the smaller group meetings were the experiments of Koffka and Koehler of Germany in relation to their "Gestalt-Psychologie." Other group discussions were devoted to problems on industrial psychology, and still others to the problems of intelligence testing. As a result of the discussions of the latter subject a committee was selected, headed by Professor Spearman, to act as a clearing house of information regarding intelligence test advancement. The hope was expressed that this committee might eventually become an important coördinating agency for European and American research on the scientific problems involved in the measurement of intelligence.

For most of the delegates these meetings gave the first opportunity since the war for establishing professional relations with psychologists in other countries, and, as usual, the benefits to be derived from a gathering of this kind are more in professional friendships rather than in the formal papers presented. To know personally an opponent in a scientific issue dispels many of the differences that appear in writing.

It was a source of satisfaction to discover that the German and the French psychologists could meet each other as scientists and as men without allowing their political differences to affect seriously the activities of the Congress. This fortunate circumstance was applauded by the audience when, for example, a German psychologist was addressing the Congress with a Frenchman in the chair.

Among the social activities of the Congress were an excursion on Sunday, July 29, to Nuneham Castle, a garden party at Worcester College at the invitation of Dr. and Mrs. William Brown, and the banquet in Christ Church College.

Among the American psychologists present were the following: Prof. Edwin G. Boring, Prof. Charles McFie Campbell, Prof. Ivy Campbell, Prof. Raymond Dodge, Prof. H. S. Langfeld, Prof. J. H.

Leuba, Prof. Adolf Meyer, Prof. Henry T. Moore, Dr. L. J. O'Rourke, Prof. W. B. Pillsbury, Dr. Morton Prince, Dr. B. Rand, Prof. Louis Leon Thurstone, Mr. E. C. Tolman, Prof. H. C. Warren, Prof. J. U. Yarbrough.

It is expected that the papers will be published in full in a volume to be issued by the committee in charge, which will include also a summary of the discussions.

On recommendation of the special committee on future meetings it was resolved that the next Congress be held in the summer of 1926. The meeting will be held in America provided satisfactory arrangements can be made for the transportation and the entertainment of a representative number of foreign delegates. If the critical condition of European finances render this unfeasible, it is expected that the next Congress will be held in Holland.

GENERAL REVIEW

REACTION-TIME MEASUREMENTS[1]

BY H. M. JOHNSON
Ohio State University

In the past decade measurements of reaction-time have been used as a means of attack on fundamental problems in nerve-physiology, in efforts to redefine the function of attention and measure its effectiveness, in tests of the influence of environmental factors, particularly drugs and lighting, and in attempts to select special personnel on the basis of supposedly valuable individual traits.

Fundamental Problems and Technique

G. R. Wells (**42**) investigated the influence of the duration of visual and auditory stimuli on the time required for simple reactions. Froeberg had previously used visual stimuli of durations varying by equal geometric intervals between 0.048″ and 0.003″, inclusive, and had found that the longest durations yielded the shortest times. Diminution of the duration by equal geometric steps produced arithmetic increments in the reaction-time. The differences are small in magnitude and their significance impressed Wells as doubtful although their statistical reliability is apparently high and their direction is consistent.

Wells used for the auditory stimulus the sound of an electric buzzer transmitted through a telephone system. The buzzer was kept continuously in operation and the chronoscope was started simultaneously with the opening of a shunt circuit around the telephone transmitter. The durations of break thus produced were 0.106,″ 0.076″, 0.051″, 0.03″ and 0.007″ respectively for the five sets of observations taken on each subject. Sufficient measurements to justify comparison were made on only two subjects, designated as *D* and *Wh*. Each gave 200 reactions under each of the five compared durations. Both were instructed to give sensory reactions.

[1] From the Department of Psychology, University of Minnesota.

The warning signal preceded the stimulus by approximately 2″. Subject *D* gave the longest reaction-time to the longest duration of stimulus, the time decreasing consistently with diminution of the stimulus until the latter reached the value of 0.03″. The difference between the reaction-times to stimuli whose durations were 0.106″ and 0.03″ respectively is about 0.009″ in absolute magnitude and is about eight times as large as its probable error. If the arithmetic values of the reaction-times are plotted against the logarithms of the durations of the stimuli the points lie very close to a straight line but *its slope is opposite in direction from the curves obtained by Froeberg.* The results obtained on subject *Wh* vary inconsistently. The subject had had no previous training.

For the visual stimulus Wells used a plaster surface whose brightness was of the order of 0.41 candles per square meter or 0.12 millilamberts. (This brightness is about one-tenth as high as that of the region at which Weber's law becomes effective.) The light-source was a Nernst filament, whose image, formed by a lens, lay along the radius of an interrupting sector attached to a pendulum. In one series of experiments reaction was made to the presentation of the stimulus, the duration of which was determined by the angular opening of the sector. The five durations used were 1.0″, 0.144″, 0.064″, 0.025″ and 0.012″. In another series of experiments on the same subjects, reaction was made to the removal of the light-stimulus, accomplished by interruption of the beam by an opaque portion of the sector, whose angular widths determined the durations. These durations were 1.0″, 0.150″, 0.066″, 0.025″, and 0.010″. The author obtained 500 measurements under each of these ten conditions on six subjects and a smaller total number on two more.

His results, when subjected to a more rigorous statistical treatment than he employed, bring out, more clearly than he seems to appreciate, the following facts: There was an optimal duration, which varied with the subjects. In 10 out of 16 cases the optimum lies between 0.025″ and 0.066″ and in two of the remaining cases the significance of the difference between these durations and the apparent optimum is doubtful. If the duration of the stimulus is either increased or diminished from the optimal value the reaction becomes slower; and the degree of retardation tends to become greater the greater the deviation from the optimal duration. The average difference in reaction-time between the optimal condition and any other condition is of the order of $0.01'' \pm 0.0013''$, for the six subjects who completed the program. In 36 cases out of 48 the

difference is greater than six times its probable error; and in 41 cases out of 48 the difference exceeds four times its probable error. In the reviewer's opinion the smallness of their magnitude is outweighed by their apparent reliability and general consistence for individual subjects.

Another interesting fact, which the author does not explain, is that for a given subject the optimal duration was often not the same and sometimes not nearly the same, for stimulus presented as for stimulus removed.

Wells asserts that his results are at variance with those of Froeberg. The reviewer suggests that Wells might profitably have distinguished between Froeberg's results and Froeberg's formula.

It seems unnecessary to question the results or to postulate, as Wells does, the influence of expectation. The formula fits the data roughly but it is too general in view of the small number of subjects and the short range of durations of stimulus which Froeberg used. The longest duration which Froeberg used was 0.048″. This lies within the optimal range for Wells' subjects, and if Froeberg had used a longer range, or more subjects, the apparent simplicity of variation might have vanished. In short, Froeberg's results can be formulated according to the law that covers Wells' results though the reverse does not hold true.

The effectiveness of a given visual stimulus for a given subject is a function, other factors being constant, of energy-wavelength distribution, area, form, brightness and duration of the stimulus; and it has been argued that if form, place of retinal stimulation and energy-wavelength distribution are approximately constant the effectiveness of the stimulus is constant within limits when the product of area, brightness and duration is constant. The function probably is more complex if more than one of these magnitudes are enlarged.

Since Froeberg's brightnesses were not measured (they probably were much higher than Wells'), and since his areas differed from that employed by Wells a direct comparison is not possible.

Furthermore, the optimal durations found for a given subject must be regarded as optimal only for the order of brightness which he used. Other variables being constant, we should expect the optimal duration to vary according to the variations of brightness. The individual differences among the subjects could be attributed to differences in visual sensitivity except for the fact that for some subjects the optimal durations were of different orders for presentation and

for removal of the stimulus. This suggests a variability in attentional attitude which complicates the matter.

I wish here to suggest for experimental attack an hypothesis which is by no means novel or original, but which, if true, will explain numerous apparent discrepancies in reaction-time results. The speed of reaction depends, first, on the adequacy of the stimulus (as to intensity, area, duration) to excite the sensory receptors; and, secondly, on the neuromuscular set of the subject. Let us call the first factor sensory and the latter "attentional," being careful to preserve our quotation marks.

If the second factor can be kept constant the reaction should be slowest when the stimulation is the least adequate, and its speed should increase with increase in magnitude of the stimulus until the stimulus becomes completely adequate. Beyond this point no effect should be found.

If the first factor can be kept constant the less the magnitude of the stimulus, the shorter the reaction-time will tend to be until a critical stimulus-value is reached, at which the reactor's effort begins to diminish.

If both factors are variable the one which in the given case is the stronger will determine the favorability of the stimulus.

To illustrate: the shorter durations of the stimulus may have been favored by G. R. Wells' subjects because these durations demanded a higher degree of muscular readiness to react than did the longer durations. In other words, the superior neuromuscular adjustment overcompensated for the less favorable magnitude of stimulation. The compensation may have been inadequate for the shorter durations. If so, we have an explanation of the generally slower response to durations of the order of 0.01″ than to durations of 0.025″ to 0.03″.

I hesitate to press this hypothesis in Wells' case because I am not wholly convinced that the shorter durations (with the exception of the minimal ones) were actually less favorable as sensory exciters. It may be that in some of his series reaction was made not merely to presentation of the stimulus or merely to its removal, but to the double excitation of presentation followed by removal, or in the other series, by removal followed by presentation. If the temporal interval between the two sensory processes excited by presentation and removal respectively was not too long the second process may have tended to reinforce the first and hence to hasten reaction. If the interval was too short, the second process may have interrupted the

first before the first process became fully effective; although it is also conceivable that the second process exerted an inhibitory effect on the first. If the interval was too long reaction might occur before the first process was affected by the second.

A better illustration of the two-factor hypothesis might be offered from some unpublished work of my own. My subjects viewed a circular test-object of foveal area and of uniform brightness save for two small spots which constituted additions of about 7 per cent to the brightness of the background. About 1.5″ after a warning signal, the right or left spot was caused to disappear and reaction was to be made with the corresponding hand. The purpose of the experiment was to test the influence on the speed of response of the degree of brightness of the surrounding field, it being assumed that the more favorable the latter was to vision the quicker the response would be. Two surrounding fields, *C* and *D*, were employed. Condition *C* was favorable because it permitted maintenance of adaptation to the brightness of the test-object and also allowed of easy maintenance of fixation. Condition *D* was unfavorable to vision because it destroyed adaptation to the test-object and rendered fixation difficult. All the subjects preferred *C* to *D* and all but one responded more quickly under *C*. This subject, in a large number of measurements, showed no consistent difference in speed between *C* and *D*, while she complained that eyestrain and general discomfort existed to an extreme degree under *D* and were absent from *C*. Crude tests suggested that this subject possessed greater visual sensitivity than the others, and I tried the expedient of reducing the brightness of the spots by half, so that they now constituted additions of only 3.5 per cent to the brightness of the test-object. This subject now found a satisfactory reaction somewhat difficult to make, even under condition *C*. The result of the change was (1) a small but significant *increase* in speed of response under *C*, and (2) a very considerable *decrease* in speed under *D*, as compared with the original conditions. In all the comparisons the probability of the difference being due to chance was quite small; and other variables than the surroundings were well controlled.

In other words, in favorable surroundings, *C*, reduction of the sensory effectiveness of the stimulus quickened the response; while in the unfavorable surroundings, *D*, the same reduction retarded response. According to our hypothesis the increased attentional strain caused by the reduction more than compensated for the impairment of the sensory factor under C, but it was inadequate to com-

pensate for the greater impairment caused by the added influence of *D*.

The experimenter concluded that in order to make the test mainly one of sensory discrimination-time, the stimuli would have to be so chosen that a maximal degree of attentiveness would be elicited by the most favorable condition under test.

Two conventional explanations of the dependence of reaction-time on intensity of stimulus meet with a challenge from Woodrow (**44**). One hypothesis, namely, that of Piéron, asserts that the time is a function of delay in excitation of the first receptive neurone. Another, namely, of Sherrington, refers the phenomenon chiefly to delay in transmission across central synapses. Woodrow points out that the two hypotheses agree in implying the concept of resistance to be overcome: they differ as to the locations of the resistances which are postulated. The exciting effect of the stimulus must exceed a minimal limit before it can arouse response and the weaker the stimulus the longer it must act before this limit is reached.

But, Woodrow reasons, suppose the reaction is made not to presentation of the stimulus, but to its removal: if Piéron's explanation is valid the weaker stimuli should arouse response more quickly than the stronger, since the excitation aroused by the weaker stimulus would require a shorter time to diminish below its threshold value. Consequently a crucial test of either hypothesis would be a comparison of the times required for reaction to presentation and to removal of a stimulus of constant intensity.

Piéron (**29**) avers that Woodrow's assumption is erroneous; that on the contrary "it is a well established fact that persistence of a sensorial excitation is longer according as its intensity is smaller; and is maximal for intensities near the threshold. On the fact that persistence is an inverse function of intensity the method of flicker photometry is founded," two surfaces being found [by direct comparison] to be equally bright when they cease to flicker at the same rate of alternation.

The exact issue between Piéron and Woodrow is not readily made out, as each author apparently implies a law of rate of establishment and evanescence of excitation which he leaves unformulated.

At any rate, Woodrow proceeded to measure simple reactions of the "motor" type to presentation and to removal of visual and auditory stimuli of varying but unmeasured degrees of intensity. The visual stimulus was the luminous gas in an arc through a Geissler's tube, and was of two degrees of intensity, designated as

"bright" and "weak." The auditory stimulus was the vibration of the diaphragm of a telephone receiver the current through which was interrupted by two forks arranged in series with each other and having vibration-frequencies of 50 and 250 respectively per second. Three degrees of intensity were used, designated as "medium, weak and liminal." The speed of reaction increased with the intensity of the stimulus for all subjects and for both kinds of stimuli.

Woodrow also asserts (p. 428) that "in the case of sound and light reactions, there is no appreciable difference between reaction-time to the beginning of a stimulus and to its cessation, no matter what the intensity of the stimulus and no matter what its mode."

From the results as thus described he argues against the concept of resistance as formulated in the hypotheses of Piéron and of Sherrington, as providing a satisfactory explanation of the nervous mechanism of reaction. He proposes that we "regard the central nervous system as not merely a network of paths but also as the seat of a complex system of interrelated activities . . . which is disturbed by a change in any part of the system." Either increase or diminution of activity in any part may serve to upset the balance of the system as a whole. A small change in excitation requires more time than a large change to produce its effects not of resistance to its conduction but because of the inertia of the preëxisting central system. "The reaction-time to light . . . is longer than for sound because of an inferior preadjustment of the cerebral mechanism."

Woodrow's results hardly warrant the sweeping formulation which he made. The differences between the average reaction-times for presentation and for removal are indeed not constant as to direction, even for the same observer (with the possible exception of subject *Vs*). However, in 7 cases out of 16 the difference varies between 3 and 11 times its probable error and the smallness of the ratios is due to paucity of data rather than to magnitude of difference. These facts suggest that the data are heterogeneous and require further examination.

In the work of G. R. Wells (42), which preceded that of Woodrow, six subjects reacted 500 times each to each of 10 visual stimuli. In one group of five sets the subjects reacted to presentation of the stimulus and in the other group of five sets they reacted to removal. The duration of the stimulus was different for the several sets in a given group, but was very nearly, though unfortunately not quite, the same for corresponding sets in the two groups.

However, it is possible to draw an interesting if not perfect comparison. Of the six subjects the one designated as *D* showed in every case a quicker reaction to removal than to presentation, the difference varying between 0.006″ and 0.0165″ and varying between 7 and 20 with respect to its probable error. Subjects *B* and *De* showed a definite tendency in favor of removal which, however, is less consistent than in the case of *D*. On the other hand, subject *Wi* reacted more quickly to presentation, the difference varying between 0.008″ and 0.021″ in absolute magnitude and between 6 and 25 with respect to its probable error. Subjects *S* and *T* also tended to favor presentation. The differences tend toward maxima for the most favorable durations of the stimulus with respect to the particular subjects.

Wells' results indicate consistent individual differences more strongly than do those of Woodrow. The reviewer is of the opinion that differences which possess these degrees of statistical reliability demand a more thorough analysis than they have received.

Poffenberger (28), interested in the effects of fatigue and narcotics (which some suppose to increase synaptic resistance), and of practice, attention and the like (which some believe to increase synaptic conductivity), attempted to develop a method by means of which the latent time of synaptic connections could be calculated. He selected simple sensory motor reactions to visual stimuli for study, and compared the reaction-times (1) of foveal stimulation with those of parafoveal and peripheral stimulation at distances of 3°, 10°, 30°, and 45° nasal and temporal. The results are quite definite and show an increase in reaction-time with increase of the separation of the stimulated area from the fovea. (The conditions of the experiment are not described sufficiently to permit of reproduction but the impression is given that the subject worked in light-adaptation.) The increase is more rapid for the temporal side than for the nasal. The curves resemble those obtained for diminishing brightness-sensitivity by Krischmann and of diminishing visual acuity, by Koester.

A further comparison was made between the times required for reaction with the right and left hands when both eyes are stimulated but no significant or consistent difference appeared. When only one eye was stimulated one subject showed no difference while one right-handed subject and one left-handed subject gave significantly shorter reaction-times with the left hand as compared with the right. All three subjects reacted more quicky, by about 0.015″, to binocular than to monocular stimulation.

The author next compared the times of reaction with each hand to foveal stimulation of each eye but found no consistent differences referable to homolateralness. This is according to his expectation since each fovea is represented in both halves of the cortex and the pathways from either fovea to either hand are geometrically equal.

However, when he subtracted the reaction-times which involved direct pathways from corresponding ones which involved indirect pathways the remainder (for the position selected for consideration) averaged between 0.005″ and 0.006″. These values he amended by subtraction of an hypothetical constant. The net remainder, about 0.004″, he regards as the time lost in the one additional (schematic) synapse involved in the longer pathway.

Henmon and F. L. Wells (**38**) challenge the assertions of Alexieff and of Whipple that individual differences in reaction-time tend to disappear with practice. They present a comparison of measurements made upon themselves in the earlier works of Henmon and of Froeberg. In this experiment both subjects had been given an unusually long preliminary training for the purpose of minimizing the effects of practice and they regard the training as effective. Nevertheless, large individual differences persisted. The average times of Wells for simple reactions were shorter than Henmon's, by 0.01″ for visual stimuli and by 0.02″ for auditory stimuli. On the other hand Wells' average times for selective reactions were longer than Henmon's by 0.04″ for colors and 0.05″ for pitch. From the independent variability of simple and selective reaction-times, the authors contend that selective reactions "are not merely simple reactions plus such processes as perception, apperception, discrimination, or choice. Hence, the times of these higher mental processes can not be secured . . . by subtraction." They also question whether the subjects of Alexieff and of Whipple had had sufficient practice to warrant the generalizations which the earlier authors had made.

Wells, Kelley and Murphy (**39**) were interested in the relation between the intensity of the stimulus and the ratio of the reaction-time to light with respect to the reaction-time to sound. In the present experiment the intensities of the visual and auditory stimuli were not measured. The visual stimulus was the darkening of a tungsten lamp. The time required for the filament to cool to a visually effective amount was not determined nor were data given which render determination possible. The auditory stimulus was the click of a relay. Thirteen subjects were used. Klopsteg's

method of timing was employed. In half the cases an interval of about 1.2″ and in the other half an interval of about 3.1″ separated the warning signal from the presentation of the stimulus. The ratios between the median reaction-time to light and the median for sound were as follows: for a group of 11 untrained observers 1.15; for subject *K*, 1.34, and for subject *W*, 1.45. The correlation between the ratios and the median reaction-time to sound in the untrained group is —0.52. Those with a quicker reaction to sound tend toward a relatively slower reaction to light. Instrumental factors are deemed worthy of consideration, however. The principal value of the study lies in the exhibition of individual differences.

The same authors in a supplementary report (40) show that in the above study the average time of the second half of a series of 216 reactions was about 5 per cent longer than of the first half. This held true of reactions to both light and sound to similar degree. This lengthening may from one viewpoint be regarded as an effect of fatigue, since it is a symptom of diminished effectiveness of response resulting from long continued repetition of the stimulus without variation of the environment. With the relatively untrained subjects this effect was much greater for a prestimulus interval of 3″ than for one of 1″. The fact indicates that the effect of monotonous work is to make it more difficult to sustain attention than to arouse it.

In a third report (41) these authors present a comparison of the same reaction-times according to the length of the preparatory interval. (The latter was irregularly varied in a given series, but always approximated 1″ or 3″.) For the group of 11 untrained reactors the average reaction-time for the interval of 3″ was about 96 per cent of the time for the interval of 1″. For the trained reactor *K* the ratio was 100, and for the trained reactor *W* 98 per cent. The range of variation for the untrained group was between 85 and 104 per cent. For reactor *K* it appeared that the average ratio for sound stimuli was about 104 per cent and for light about 96. The speed of reaction for the untrained group showed a small negative correlation (—0.22 to —0.27) with degree of preference for the longer interval. For reactor *K* there was a positive correlation of 0.3 for all reactions combined and of 0.5 for reactions to sound. For reactor *W* there was no correlation.

Applications to Problems in "Attention"

Among the most fruitful studies to which reaction-time measurements have ever been applied are the more recent investigations of Woodrow (43), (44), (45), (46).

In the first study (43) which is aimed at securing a measure of attention, Woodrow argues that attention is a process which, while it has not been satisfactorily defined, is known by its functions. One of these functions is efficiency of response, so that the measure of such efficiency constitutes an index of variation in attention. The particular response which the author selected is the simple reaction and he applied his measure of "efficiency" to the reaction-time under various conditions of distraction and detraction.

In the first set of experiments the "motor" form of simple reaction was made to stimulation of a Wundt sound-hammer. The variable factor was the temporal interval between the preparatory signal and the presentation of the stimulus. This interval varied between 1″ and 32″. At each sitting the subject reacted 25 times with each interval, the latter being given in the same temporal order as their magnitudes. Three subjects were employed and a total number of 125 to 150 reactions obtained for each interval. The results show that an interval between two and four seconds is the most favorable, an increase in reaction-time resulting from increase or decrease of the preparatory interval outside this range. Only one value smaller than 2″ being used, no attempt was made to formulate the relationship between diminution of the interval and increase of reaction-time. For larger intervals than 2″, reaction-time tended to increase as the logarithm of the interval.

The original experiment was now varied by the use of an irregular order of interval within each series of 25 reactions. The original three subjects were used, with the result that the optimal interval was shifted to the region between 12″ and 16″. The differences showed much less variation with variation of the preparatory interval than in the case of the "regular" order.

From these results the author concludes that the use of irregularly mixed, or of unfavorably long preparatory intervals is a satisfactory detractor of attention. He was not interested in the effects of unfavorably short intervals. He now wished to compare the detractional effect of the unfavorable interval for stimuli of variable intensities. For this purpose he discarded auditory stimuli in favor of visual stimuli since the intensity of the latter is more amenable to

control. The subject, in a dimly lighted room, viewed by the aid of reflected light a fixation mark on a ground glass screen on which an image of variable brightness was projected by means of a stereopticon and viewed by transmitted light as an addition to the brightness of part of the screen. Exposure was effected by a Seashore tachistoscopic shutter. Simple reaction was made to the presentation of the stimulus. Four image-brightnesses were used, ranging between "very dim" and "bright." These brightnesses were not measured, and the omission prohibited the establishment of a quantitative relalationship between the effectiveness of the detractor and the relative intensity of the stimulus. The author had to content himself with ranking the different brightnesses and plotting separate curves for each. The different preparatory intervals were used in the temporal order of their magnitudes. Two subjects were employed, one being the author. He formulates the dependence of reaction-time, y, on the length of the preparatory interval, x, between x-limits of $2''$ and $24''$,

$$y = A + B \cdot \text{Log } x,$$

A and B representing constants whose magnitudes, though different for different subjects, are inverse functions of the intensity of the stimulus. This relationship fails at the lower limit of $2''$. Had the study included a wider range of intervals a general formula would doubtless have exhibited the function as complex or discontinuous. The manner of plotting does not exhibit this fact as well as a graph of time against the logarithm of the interval, but the author was clearly aware of it.

Woodrow's next experiment was made partly for the purpose "of determining whether it is possible to find a form of reaction the time of which is unaffected by variations in retinal sensitivity and so to get a method of measuring attention apart from its dependence on retinal sensitivity." For this purpose he employed selective reactions to the darkening of one or two rectangular patches of light projected on to a screen. He used four "widely different" absolute brightnesses of these patches, designating them respectively as medium, weak, weaker and very weak, but did not measure them. The darkening was accomplished by the insertion of an absorptive screen in one of the two paths of light at or near its smallest diameter by means of a tachistoscope. The same screen was used throughout. Its transmissivity was not measured; but its effect was described as being "noticeable without the least hesitation." Reaction consisted of speaking the word "right" or "left" (according as

the right or left patch was darkened) into a voice key, which served to stop the chronoscope. A constant preparatory interval of 2″ was employed. The reaction-times for the various absolute brightnesses (with the exception of the lowest) present rather small differences—a result to have been anticipated within the range to which Weber's law applies.[2] The author concludes that this type of experiment is suitable for testing the effect of other detractors on attention.

Woodrow now proceeded to test the effect or reaction-time of two detractors: namely, unfavorable changes in intensity of stimulus, and irregularity of preparatory interval. For this purpose he used three rectangular patches of light whose brightnesses were constant and mutually equal, though unmeasured. The stimulus to reaction was the darkening of one of the three patches to variable extents, by interposition of one of five absorptive screens in the appropriate path. The subtractive effects of the several screens were not measured but varied between "small" and "large." Reaction consisted of speaking the word, "right," "middle" or "left" as the case might be. In one set of experiments the preparatory interval was 2″. In the compared set the interval was irregularly varied. Two subjects were used.

The results showed (1) under both conditions that the greater the darkening the shorter the reaction-time; but the effect was far greater for the irregular preparatory intervals than for the interval of 2″, (2) the use of the irregular interval increased the reaction-time by amounts varying between 0.1″ and 0.22″ and the difference was greater the smaller the darkening.

In the reviewer's opinion the technological value of these findings is very great despite the author's failure to make possible a duplication of the experimental conditions or the establishment of quantitative relationships between magnitude of stimulus and effect of detractors. The report includes an exhibit of the influence of practice on the detractional effect of irregular preparatory intervals.

In another study, Woodrow (45) required his subjects to react, as quickly as possible, to the darkening, the extent of which was

[2] A fact which is interesting although its interpretation is not obvious, is that the absolute times required for selective reaction to these stimuli are in the average between 30 per cent and 80 per cent longer than those obtained by the reviewer under the most favorable conditions of vision. In this work the stimulus was a difference in brightness lying between 4 per cent and 5 per cent; the preparatory interval lay between 1.0″ and 1.5″; and the reaction keys were operated manually.

unmeasured, of an illuminated spot, whose brightness was also unmeasured, under two conditions: In the first the preparatory interval was 2″; in the second this interval varied irregularly during the sitting between 4″ and 20″. The reaction-time was uniformly longer under the second condition than under the first and the difference between the averages was taken by the author as the inverse of the degree of attention and designated as 1/A.

Two such sets of experiments were run concurrently. In one the brightness of the spot diminished from an effective value at the center to a sublimal value at the edge but the gradient was so slight that a definite boundary could not be determined. In the other set the same spot was limited by a diaphragm placed between the projector which formed it and the screen, so that there was a definite difference in brightness between the edge of the spot and the background and hence the spot had a definite outline. The effect of the unfavorable preparatory interval was of much greater magnitude in the first set than in the second, from which the author concludes that the outline elicited a higher degree of attention. The magnitude of the difference of the second order varied between 28 per cent and 40 per cent, according to the subjects, and for a given subject it was a function of the degree of change in brightness to which reaction was made. All the subjects reported that the change in brightness was clearer and easier of observation when the outline was visible than otherwise.

In still another study (**46**) Woodrow compares the magnitudes of these second-order differences in simple reactions to light, sound and touch, of "moderate" but unmeasured intensities. The average effect was about 11 per cent greater for sound than for touch and about 11 per cent greater for light than for sound. These values the author regards as expressing the differences in degree of attention to the three modes of stimuli. The correlation between degree of attention to any two modes of stimuli was practically unity even when corrected for differences in ages of the twelve subjects, of whom five were young children. The correlation between age and degree of attention was found to be approximately 0.8 and constant for the several modes.

A comparison was also made between the second-order differences for simple reactions to light and for selective reactions. The latter showed an effect about 60 per cent greater than the former, in the average, which indicates, according to the author's definition, that "attention" in a discriminatory reaction is lower than in a

simple one. Some readers may find difficulty in following the argument by reason of difficulty in holding to the author's use of the word "attention."

The latter finding would seem to suggest that the most fruitful method of using reaction-time measurements as an index of the effect of a variable is to employ selective reactions to visual stimuli with and without detractors, with the tested variable present and absent. The reviewer feels that Woodrow's study, by reason of lack of measurement and comparison of intensities, fails of complete proof of this proposition, but establishes a considerable presumption in its favor.

Evans (13) employed nine subjects in simple reactions to visual, auditory and tactile stimuli, with visual, auditory and tactile distraction stimuli. The visual stimulus was a circular field whose diameter subtended about 1.1° at the eye, illuminated by a Geissler tube the radiation from which passed through a yellow filter. The field was seen in fairly dark surroundings and its brightness was not measured nor does the author's description permit of estimation of its order of magnitude. The sound stimulus was the impact of a Wundt sound-hammer on its anvil. The tactile stimulus was a Cattell and Dolley touch key. The visual distractor was a field similar to the stimulus field but located slightly above it. The auditory distractor was a second sound-hammer whose effect differed from that of the stimulus in quality and loudness. The tactile distractor was a weight which fell upon the back and side of a finger.

All the distractors lengthened the reaction-time, whether measured in gross averages or in daily averages and also increased the subjects' variability. The effect was greatest when the distractor was first introduced; it was diminished by training, but was never entirely overcome. The effect held for trained subjects as well as for novices. The distractor influenced the control sets as well as the sets in which it was present. Sound proved more effective than light and light more effective than touch. The effect of a distractor was greatest on the reactions to stimuli belonging to its own mode. Training in simple reaction to one stimulus shortens the simple reaction to another stimulus. Training in reacting without distraction does not appreciably aid in reacting to the same stimulus with distraction. Training in reacting with one distractor reduces the initial effectiveness of another distractor, and shortens the period of adaptiveness to it.

Cassel and Dallenbach (8) note that most frequently distracting or intercurrent stimuli are reported as lengthening the time required

for reaction; but that sometimes the contrary effect is found; while occasionally, after a brief initial disturbance, they leave the time unaffected. Two subjects completed the required course of duty. Reaction was made by releasing a telegraph key. The stimulus was the noise of a Wundt sound-hammer, adjusted so that only the impact of hammer on anvil was audible. Time was recorded by means of a Hipp chronoscope. Two flashes of a Geissler tube serving as preparatory signals were given in succession 3″ and 1.8″, respectively, before the stimulus. The subjects were instructed to give "sensory" reactions and to record their experiences after each group of ten reactions.

Three distractors were employed: (1) a metronome whose beats were separated by 0.5″; (2) an electric bell, which rang continually for 2.5″, beginning 1″ before the stimulus was presented; and (3) an electrically driven tuning fork whose frequency was 256 v/s. The daily sitting began and ended with a series of 10 normal reactions; and between these two series 10 series with a particular distractor were given.

When the data were assembled it was found that both observers required a longer time for reaction under distraction than without distraction, by amounts ranging between 0.003″ and 0.037″, depending on the subject and the distractor. Subject *R* was more affected than subject *D*, and for both observers the order of effectiveness of the distractors was bell, metronome and fork. All of the differences but one are large with respect to their probable errors. The variability of the measurements varied in the order of their magnitudes.

However, when the data were fractionated, it was found that they were heterogeneous; for on some days the reactions were quicker under distraction than otherwise and the probability that such differences were due to chance was small. That the effect in the average was in the opposite direction was due to the fact that on the majority of days the distractors inhibited rather than facilitated reaction.

An explanation was found in the reactors' own reports, which led the authors to conclude: (1) that the effect of a distractor on simple sensory reactions is not constant in direction; (2) that this effect is dependent on the temporal character of the distractor, on the "conscious attitude" of the observer during the distraction; (3) that intermittent distractors are most resistant to inhibition and continuous distractors the least; and (4) that the passive attitude is conducive to a constant sensory reaction of normal length; the active

attitude to a slow and variable reaction. The authors believe that the disagreement among earlier students has been due (1) to the employment of distractors of different temporal characteristics, (2) to neglect of control of the reactor's attitudes, and, (3) to the basing of conclusions merely on gross averages. The contribution, in the reviewer's opinion, is important.

In the above experiment the reactor Dallenbach, an expert in this difficult type of introspection, estimated the "clearness" of his sensory content at the time of reaction. At first these judgments were made after each series of 10 reactions; but in the last 700 cases they were rendered immediately upon each individual reaction. In a later study (9), the authors segregate the reaction-times of this reactor according to their degrees of "clearness." The greater the clearness reported the shorter the reaction-time tended to be, and the smaller the variability. One immediately wonders how strong this tendency is.

The authors present coefficients of "correlation" between *average* time and clearness which vary according to the distractor between [—] 0.913 and [—] 0.999; and between mean variation and clearness (in the case of 700 measurements) of [—] 0.97. From these figures they reason that under the experimental conditions and for their introspective reactor, "attributive clearness may be measured by the average duration and mean variation of the simple sensory reaction." From the work of Geissler they derive the proposition "attention can be measured in terms of attributive clearness." Hence they conclude that these measurements "give a reliable index of attention."

Furthermore, in the main study, it was found that Dallenbach's attitude "changed frequently during the course of the experiment, and also that the reaction-time varied with attitude. The present experiment shows that *reaction-time is closely correlated with clearness.* It would therefore appear that clearness and attitudes are interrelated." (Italics mine.) The introspective findings might, of course, be challenged. Reaction certainly tends to become quicker with practice; and, as in other instances of habit-formation, some reactors, including myself, have incidentally noted a tendency for the degree of consciousness of everything connected with the response to diminish, and almost to vanish, with continued practice. Of course, as a behaviorist, I could not gracefully urge this evidence even if I had systematically recorded it; and besides, the authors

disclaim the application of their law to the results of untrained introspectors.

We may, however, inquire whether or not the introspective data, taken at face value, really support the authors' reasoning. We shall presently see that they do not. The coefficients presented would indeed support it if they expressed correlation between clearness and, for example, time. That is to say, if they were obtained by treating each paired measurement as a single datum, and basing the computations on the deviations of these individual measurements from their means. The figures presented were not obtained in this manner but, as the authors also say, are based on the weighted averages of reaction-times distributed according to degrees of clearness. Such treatment disregards the variations of time corresponding to each degree of clearness, and the variations of clearness corresponding to each value of time; these being the very data from which instructive correlations are derived. (Such treatment, if there are no reversals in the regression-curve, will give a result approximating to unity even though the actual correlation between the two variables is insignificantly small. The result is therefore irrelevant.)

In the present instance the authors give: (1) the frequency-distribution of the values of clearness; (2) the averages of time for the several degrees of clearness; and (3) the mean variations from these averages. From (1) we may directly obtain the standard deviation for clearness (σ_c).

From (3) we may approximate the standard deviation for time (σ_t') if it be granted that the standard deviation (σ_t') from each distributed mean (M′) in their table is equal to 1.25 times the mean variation from M′, as in a normal distribution. In such case the standard deviation (σ_t), from the general mean would be

$$\sigma_t = \sqrt{\frac{1}{N} \cdot \left(\Sigma \left[p \left\{\sigma_t'^2 + \overline{M' - M}^2\right\}\right]\right)}$$

in which p represents the number of measurements included in each of the averages of the distributed times, M the general mean, and N the total number of measurements.

From (2) we may get by least squares the equation of the straight line which best fits the weighted averages of time when plotted against clearness. The slope of this curve is by definition the coefficient of regression ($\rho_{t/c}$) of time on clearness. The necessary relationship

$$\rho_{t/c} = r\frac{\sigma_t}{\sigma_c} \text{—yields } r = \rho_{t/c}\frac{\sigma_c}{\sigma_t}$$

and by substitution we may get a numerical value of r which is as reliable as the assumed relationship between MV and σ_t'.

Applying this procedure to the last 700 measurements, for example, we find that the true product-moment correlation r between time and clearness is approximately —0.25; and some such value as this should have been used instead of [—] 0.992 as the basis of the author's argument.

Far from supporting their proposition that under the conditions of the experiment, "the rate of a simple sensory reaction [is] a reliable means of determining the degree of clearness," the data indicate that under the specified conditions, reaction-time as a means of determining the degree of clearness is approximately 3 per cent more reliable than guesswork. The various conclusions drawn by the authors fail with this premise.

Application to Studies of the Effects of Drugs

Hollingworth (19) used selective reactions to colored cards to test the effects of caffein. At the beginning of the day's work a sitting was held to establish the subject's normal for that day. A dose of caffein, or in its stead, of lactose, was later administered and the test was repeated. The size and form of the dose and the time between dose and test were varied systematically. The results are presented in the form of averages of groups of subjects. The author concluded that small doses of caffein retard discriminative responses and increase the number of errors. Larger doses mask this effect by reason of their stimulatory influence. A considerable time after the large doses retardation may occur. Small doses of caffein if taken with the noon meal produce less retardation than if taken alone. The magnitude of the effect is a function of body-weight.

Macht and Isaacs (25) tested the effect of a number of opium alkaloids on the time required for simple reaction to spoken words, to pressure and to the sudden illumination of a white screen, and also, on the time required for simple operations in mental arithmetic. Tests were made before and after administration of the drugs but no report is made on comparable doses of innocuous substances. They found an effect on either the averages or on the variability or on both, depending on the size of the dose and on the temporal separation of dose and test. After small doses of morphin, there is generally a primary stage of excitation with shorter reaction time; and this stage may or may not be followed by a secondary stage of depres-

sion with increased reaction-time. After larger doses of morphin the primary stage is very short and may be overlooked, whereas the secondary effects are likely to be more pronounced. Morphin, when combined with other opium alkaloids, as in narcophin and pantopon, is more depressant than when the same dose of morphin is given alone.

Macht, Isaacs and Greenberg (**26**) extended the study to the effects of quinin, acetanelid, phenacetin, antipyrin, salol, aspirin and pyramidon, singly and in combination. All except quinin clearly tended to retard reaction-time, the less complex processes being the more affected. The effect on the arithmetic operations was sometimes increased speed but diminished accuracy. This effect is contrary to that of the opiates, and in the authors' opinion suggests that the antipyretics act on lower synapses than do the opiates. Combinations of antipyretics give simple additive rather than synergistic effects.

Applications to the Study of Lighting

Johnson (**20**) compared the quickness of selective reaction to the darkening of the right or left half of a photometric field by approximately 4.6 per cent under three lighting conditions. Under condition 1, the field was viewed in surroundings which were about 0.75 time as bright as the test field. Under condition 2, the surroundings were too dark to be measured. Under condition 3, the surroundings were 2.25 times as bright as the test field. The test field itself was of foveal dimensions; its brightness was approximately 7.8 millilamberts, and its color was approximately that of a black body at a temperature of 2400° K.

About 18,000 measurements were made on four thoroughly practiced observers, with the following results: All the subjects required more time for reaction in dark surroundings (condition 2) than in moderately bright surroundings (condition 1). The magnitude of the differences ranges between 0.01″ and 0.06″, or between 2 and 20 per cent, among the several reactors and in seven sets out of eight the difference ranges between 11 and 40 times its probable error. All the subjects required more time for reaction in surroundings 2.25 times as bright as the test-object than in surroundings 0.75 time as bright. The magnitude of the difference ranges between 0.01″ and 0.03″, or between 4 per cent and 10 per cent, among the several subjects, and is between 10 and 22 times its probable error. Three of the four subjects showed greater retardation due to dark

surroundings than from excessively bright surroundings, the fourth subject giving one set in reversal and one set ambiguous.

All the subjects showed greater variability in response (as indicated by the standard deviation) in dark surroundings than in moderately bright surroundings. The magnitude of the difference varies between 15 per cent and 54 per cent among the different subjects and is between 5 and 17 times its probable error. All the subjects showed greater variability in excessively bright surroundings than in moderately bright surroundings by amounts varying between 11 per cent and 44 per cent and being, in seven sets out of eight, between 8 and 15 times their probable error. One subject showed twice as great variability in dark surroundings as in excessively bright surroundings; for the other three the differences are small and of doubtful significance.

Three of the four subjects made a much smaller number of errors in moderately bright surroundings than in surroundings that were dark or excessively bright. The fourth subject showed no important difference and made a very small absolute number of incorrect reactions.

All the frequency-distribution curves obtained were multimodal, the position of the modes being stable and independent of the unit of measurement of the measuring instrument, and nearly independent of the total number of measurements included in the distribution after the number exceeded 200. Their position tends toward constancy for different subjects. The principal difference between reactions under favorable conditions and under unfavorable ones lies in the relative height of the several modal ordinates. The author suspects that there are a limited number of reactive mechanisms, having different latent times; that those having the shorter times are the harder to excite and hence are less frequently excited under unfavorable conditions than the others. In another paper (21) he asserts that the effect of training in reaction-time work consists largely in increased frequency of use of the more quickly acting mechanisms over the others.

Johnson's major study (20) was made primarily for the purpose of determining whether the measurement of reaction-time measurements can be used advantageously in testing moderate differences in the distribution of lighting and reaches a favorable conclusion. Subsequent experimentation, as yet unpublished, has confirmed this view,

but has also exhibited the need of additional precautions to minimize the effects of compensatory effort on the part of the subject.

Elliott,(12) working in a laboratory maintained by the manufacturers of the mercury-vapor arc lamp, obtained comparative reaction-times which she interprets as representing the time required for cognition of printed four-place numbers, with the test-object illuminated by mercury-vapor, tungsten and solar light of different intensities. The manufacturers of the mercury-vapor lamp, in their advertising propaganda, have long asserted that this light is superior to the light yielded by incandescent sources in that (1) it affords a higher visual acuity; (2) it is more effective at low brightnesses of the visible surfaces; and (3) more of it can be obtained for a given consumption of power. More recently it has been asserted (4) that visual response is quicker under mercury-vapor light than under light from incandescent sources. The last mentioned claim is the subject of Elliott's investigation.

Her subjects were three: a salesman, an accountant and a stenographer, whose defects of refraction were presumably corrected by glasses. The stimuli were four-place numbers, printed on "white" paper tape, presented behind a hole in a colorless screen and concealed by a colorless shutter. The reflectivity of none of the surfaces is given. A chronograph was started when the stimulus was exposed and was stopped by the subject's pressing a reaction-key to indicate that he had "cognized" the number. The actuation of the key caused the shutter to conceal the stimulus. After the subject had reacted, he pronounced the number to the experimenter, and if the response was correct the fact was taken as evidence that the subject had cognized the number at the instant he pressed the key or at a constant interval theretofore or thereafter. This assurance appears to have come through the channels of faith. To those lacking this medium of knowledge the weakness of the assumption would tend to invalidate any results which might be based on it. The use of an apparatus in which reaction is registered vpcally by means of a voice—or lip-reaction-key, actuated by the subject in pronouncing the number, would have lent greater weight to the results. The experimenter, in considering a list of extraneous variables which she sought to render ineffective, remarks:

"Preference for any one kind of light might affect results, but in this case the effects would necessarily be very marked, and would undoubtedly appear as wide and erratic differences in the reaction-

times. The fact that no such fluctuations appeared but on the contrary the recorded times show a remarkable consistency, is sufficient evidence that they were not subject to the influence of prejudice."

The reviewer takes exception to this assertion, without, however, implying that prejudice was actually operative. If two situations are objectively equal, and the subject's neuromuscular set or attitude or purpose is different in the two cases, the fact may express itself in a consistently unequal preparedness for reception and for reaction. The result might be two sets of reaction-times whose averages are unequal but whose dispersions are similar. Control of this factor of so-called "voluntary attention" or neuromuscular set is as important as control of the environmental situation. Not only should its character be ascertained but comparative records should be obtained from subjects whose preferences with respect to a given condition of observation are positive, negative and neutral. This criticism is offered in the belief that it is severe, but without the slightest intent of questioning the good faith of either experimenters or subjects.

The reaction-times under mercury-vapor light were uniformly shorter than under solar light of the same intensity. The absolute differences varied between values of the order of 0.01″ to 0.04″, according to the different observers. The reaction-times under mercury-vapor light are uniformly shorter than those under tungsten light of the same intensity. The difference varies between 0.02″ and 0.08″, being greatest in the case of the salesman, and, except at the lowest illumination, tending to increase as the illumination is increased. The probability of the differences being due to chance is in only one case as great as one in 10,000. The reaction-times under solar light are shorter than under tungsten, but the differences are less than those between mercury-vapor and tungsten. Reaction-times under all conditions diminished rapidly as illumination was increased until a critical value was reached. This limit lay near 5 to 10 foot-candles for all subjects but one. For this subject the limit was higher, but his results were irregular. Increase of illumination beyond the critical value resulted in changes in reaction-time whose magnitudes are insignificant and whose direction is usually reversed.

If one disregards the defects in the procedure and the invalidity of interpretation of the results which were noted above, and assumes that the speed of reaction is an index of vision, one may say that according to individual subjects, an illumination of 0.5, 2 or 7 foot-

candles in mercury-vapor light is as effective as an illumination of 50 foot-candles in tungsten light, under the conditions of the tests. That some such commercial interpretation will be made of the results has already been foreshadowed in articles (11) on factory lighting in recent industrial journals. Whether this be the fact or not, the reviewer feels that the present experiment has not established it, and that the consequences of such a conclusion are important enough to demand caution.[3]

Applications to the Selection of Personnel

Attempts were made in the air service of the French, Italian and American armies to use the speed of simple reaction as an accessory means of diagnosing aviational ability among candidates for training.

Camus and Nepper (7) present a preliminary report, which was inaccessible to the reviewer. According to Dockeray and Isaacs (10) only illustrative records are given. It appears, however, that the candidate was required to react ten times each to a visual, auditory and tactile stimulus; the average times were measured and those subjects regarded as ineligible whose averages exceeded by more than 0.1″. The subjects were also tested before and after being startled by a pistol shot or other harsh, loud and sudden noise; and those whose reaction-time was abnormally lengthened by the emotional state were given an unfavorable rating.

The general methods employed in the Italian army have been described in greater or less detail by Gemelli (15), (16), (17), and Gradenigo (18).

According to Saffiotti (33) the candidate was required to give 20 simple reactions to light and 20 to sound after a very brief preliminary practice. Averages most frequently obtained varied between 0.17″ and 0.20″ for light and between 0.13″ and 0.15″ for sound. Candidates whose averages exceeded the upper limits were unfavorably rated; as were those whose results exhibited a standard deviation greater than 0.03″.

Saffiotti also reports briefly another test much like the one described in greater detail by Romagna-Manoia (32), in which the

[3] In the figure shown by Elliott as representing the wave-length distribution of radiant energy evaluated according to visual effectiveness, for different light-sources, the curve labeled "Tungsten light" should be labeled "Skylight"; while the curve labeled "Sunlight" should be labeled "Tungsten filament" (22 lumens per watt). Cf. Luckiesh, *Light and Shade*, etc. New York: Van Nostrand, 1916. P. 70.

subject was required to move a lever in one of four directions in response to one of four visual stimuli and to inhibit response to a fifth. The average times obtained on 400 candidates between eighteen and twenty-two years of age varied between 0.33″ and 0.66″, the mean being 0.47″; the average deviation, 0.06″; the average coefficient of variability, 0.12″; and the average percentage of error 0.7 except for failures to inhibit response, which occurred in about 2.2 per cent of the cases. Only 12 reactions to each stimulus were timed.

Neither Saffiotti or Romagna-Manoia say what use was made of these results. Azzi in a brief note (2) refers to Saffiotti's work and says that average times of 0.68″ to 0.70″, average deviations of 0.22″, coefficients of variability of 0.36″, and percentages of error of 26 are abnormally high for pilots.

Magis reports some results obtained under the supervision of Herlitzka on the effect of an emotional state on reaction-time. A series of simple or selective reactions to visual or auditory stimuli were measured, following which the patient was excited by an unexpected pistol shot or similar noise; whereupon a second series of measurements were obtained. The earlier measurements of the second series were usually much longer than the normal series and contained more errors; but there was usually a gradual recovery. If the median of the second series exceeded that of the first by less than 10 per cent the diagnosis was "good"; if by 10 to 25 per cent, "mediocre"; if by more than 25 per cent, "inept" ("*inabili*"). Although these limits are somewhat arbitrary their choice is colored by the fact that patients whose hardships had rendered their "nervous" condition clearly pathological showed the greatest effect.

Saffiotti (p. 183) asserts that simple reaction-time under normal conditions showed a definite lengthening as a result of the severities incident to the work of pilots and that the progress toward recovery was readily paced by the improvement. In view of the results obtained by American experimenters on drugs, fatigue, etc., the findings may well be received with caution; since a much more highly developed technique, such as that of Woodrow, seems usually necessary to the obtaining of unambiguous results.

Stratton, McComas, Coover and Bagby measured a small number of selective reactions made with airplane controls to multiple stimuli by a number of cadets, who were rated as to aviational ability by

their trainers. The correlation (0.26) between aviational ability and speed of reaction was practically insignificant; that between speed and inaccuracy being somewhat higher (0.39). The low correlation may possibly be explained by (1) lack of differentiation by the trainer's ratings, (2) lack of sufficient practice in the test; and (3) the fact that the controls were very clumsy so that the largest and most variable portion of the total reaction-time was that involved in their manipulation.

Dockeray and Isaacs (10) measured the time required for simple reactions to visual, auditory and tactile stimuli by monitors and by fit and unfit chassé pilots in the A. E. F., and found an insignificant correlation between the speed of reaction and aviational ability as estimated by the training department.

The present allotment of space does not permit of a summary of the work on association-timing, or of the analytical work of Piéron. The latter may be treated later in a special review.

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SYZMANSKI, J. S.; KÖHLER, W.; VON FRISCH, K.; and BRUN, R. *Methoden der experimentellen Psychologie: Vergleichende Tierpsychologie.* (Abt. VI, Teil B, Hf. I and II of *Handbuch der biologischen Arbeitsmethoden.* Ed. by EMIL ABDERHALDEN.) Berlin and Vienna: Urban & Schwartzenberg, 1921 and 1922.

The contents of the first number of this projected volume are made up of an article by J. S. Syzmanski on General Methods of Comparative Psychology and one by Wolfgang Köhler on Methods for the Psychological Examination of Apes. In the second number, K. von Frisch presents an account of the Psychological and Sensory-physiological Methods for the Study of Bees, and Rudolf Brun discusses the Psychological Investigations of Ants. Eight additional contributions are promised by the editor to complete the volume on Comparative Animal Psychology. The titles of these are the psychological studies of horses, dogs, other mammals, birds, reptiles and fish, the learning process in lower vertebrates and invertebrates, sense-perceptions of animals, and the methods of animal hypnosis. The authors of these studies are Pfungst (2), Henning (3), Buytendyk (2), and Mangol (1). The volume when completed will afford a very comprehensive survey of the field of animal psychology and, in this respect, will be reminiscent of the work begun by Gustaf Kafka in his Tierpsychologie, 1914.

In the present numbers the discussions are based upon summaries of experimental work accomplished in the respective fields. American work is referred to extensively, although even in Syzmanski's discussion there is no direct treatment of behaviorism. All four of the writers discuss methods and results from the standpoint which assumes the desirability of understanding the subjective life of animals. Köhler places great stress upon qualitative procedures and devotes little space to the discussion of the usual experimental problems. Chief emphasis is placed upon the examination of intelligent behavior. This type of response involves the use of intermediary objects for the attainment of a goal which cannot be directly reached. Thus the experimenter arranges a situation so that, in order for the

animal to secure food, a box must be moved and then be climbed upon. The primary condition of such an experiment is that all of the components of the situation shall be within the animal's range of vision. Köhler adds little if anything to the discussion of this method which we owe to Thorndike and Hobhouse. He speaks of intelligent behavior as insight behavior, but he fails to see that "insight" may be present in any type of response. The scientific problem is to devise a task which can only be solved by "insight," *i.e.*, by the "perception of relations." So far as the reviewer knows, this has never been done and probably cannot be done. If, therefore, we are to discuss intelligent behavior, it cannot be in terms of the presence or absence of insight. I doubt if agreement could be reached upon a common term, such as intelligent, to designate the various forms of behavior which seem relevant to this type of response. Rather we must adhere to such individual terms as language behavior, highly variable behavior, etc.

Syzmanski's survey of general method, couched as it is in outline form, is well calculated to give the reader a broad view of problems and techniques. The discussion is organized under the following headings: primary, secondary and tertiary (intelligent) behavior, the analysis of impulse, and the examination of expressive movements. Inasmuch as detailed presentations of researches are not attempted, the account suffers from the consequent separation of methods and results. Keen criticism is directed throughout the study against uncontrolled experimental work, of which Köhler's tests for intelligent behavior are one sample.

The studies by v. Frisch and Brun on bee and ant behavior are not so much methodologies as they are summaries of the present status of the two subjects. Both accounts are well illustrated with figures portraying experimental conditions. Both are models of scientific exposition.

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BUNDY, W. E. *The Psychic Health of Jesus.* New York: Macmillan, 1922. Pp. xvi+299.

The presentation of Jesus as an insane person (epileptic, paranoiac, ecstatic) is a recent outcome of the insatiable curiosity shown for the founder of the Christian religion. The author of this book, a student and teacher of the Bible, summarizes the pathographic literature and attempts to determine the degree of its correctness.

After a chapter upon "The Earlier Stages of the Problem," six works, the oldest one of which is twenty years old, are reviewed: *War Jesus Ekstatiker,* by Oskar Holtzmann; *Jesus: Eine vergleichende psychopathologische Studie,* by Emil Rasmussen; *Jesus Christus von Standpunkte des Psychiaters,* by Dr. de Loosten (a pseudonym for Dr. Georg Lomer); *Religion and Civilization: the conclusions of a Psychiatrist,* by William Hirsch; *La Folie de Jésus,* by Dr. C. Binet-Sanglé; and *Die Gemuetsart Jesu,* by Julius Baumann.

The incidents in the life of Jesus and the traits of character that have been regarded as symptoms of insanity are mainly his hallucinations (at the Baptism, in the Desert during the Temptation, at the Transfiguration, at Gethsemane); certain violent and, it is said, senseless acts (the cursing of the fig-tree, the cleansing of the Temple); his indifference to his family and to the sex relation; and, most of all, his assumption of the rôle of Messiah and his consequent extravagant egocentric attitude.

The argument in rebuttal offered by the author is threefold: (1) The sifting of the sources upon which the judgment of insanity is based. First of all and in agreement with modern scholarship, the Fourth Gospel must be rejected. That Gospel puts in the mouth of Jesus convictions and confessions that were those of early Christians and not necessarily his own. Secondly, the Johannine and other apocryphal elements in the synoptic Gospels must be eliminated. When that is done, the foundations upon which the authors mentioned rest their case crumbles to almost nothing. (2) In order to be understood correctly, the apparently incriminating words and actions of Jesus must be placed in their historical setting. In several instances they, then, cease to appear abnormal or at least they lose much of their virulence. The belief of Jesus in his Messiahship assumes, for instance, a different cast when it is realized that the expectation of a Messiah was prevalent at the time and that, therefore, a belief which to-day might be regarded as a sign of insane credulity could then be entertained by sound minds. (3) One cannot, from a few hallucinations or from detached and incomplete reports of other events, conclude or infer epilepsy or paranoia. In order to do so safely, one should be able to follow the developmental course of the hallucinations or delusions and to relate them to the general situation and behavior of the individual. Jesus suffered a

number of hallucinations but that in itself does not constitute insanity. They did not dominate or even materially influence his life.

The argumentation of the author carries conviction. When the sources have been sifted and the remaining evidences placed in a setting belonging to them, that which remains is entirely insufficient to substantiate the diagnosis of either epilepsy or paranoia.

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ELLWOOD, C. A. *The Reconstruction of Religion—A Sociological View.* New York: Macmillan, 1923. Pp. xiii+323.

In this book a distinguished sociologist who has reflected long and earnestly upon the course of humanity and who has been deeply moved by the failure of religion made manifest during the last years, has put all his anxieties, his hopes, and his most mature knowledge. It deserves attention in this JOURNAL as an application of sociology to the problem of social construction or reconstruction.

The outline of the book is easily traced: Humanity, even the theoretically Christian part of it, has not transcended paganism and is, in consequence, threatened with decomposition. In order to overcome its evils, society needs scientific direction and religious inspiration. As it exists to-day, Christianity is estranged from science and to some extent from democracy, and is, therefore, not in a position to save humanity. But a Christian religion purified and simplified until in agreement with the teachings of Jesus himself would be adequate to the task. The thesis of the book is that only a Christianity transfused with the spirit and transformed by the method of modern science would be equal to the task of saving modern civilization (p. viii).

The expression "positive Christianity" is used to designate what the author regards as the religion of Jesus in distinction from historical Christianity. That religion minimizes theology and metaphysics, is concretely ethical, collective, active (not merely contemplative), and coöperative, *i.e.*, it is social in the full meaning of the term.

The doctrines of this positive Christianity are to be limited to the affirmations of: (1) A God "who manifests himself in nature, not only as creative evolution, but in human nature and in human society as the spirit leading towards all truth, all rightness, and all brother-

hood" (138). This immanent conception replaces the Santa Claus, the autocratic, and the materialistic conceptions of God. (2) Personal immortality. In what precise form this will be realized "will not concern positive Christianity," but "there is nothing in positive science which forbids a reasonable faith in personal immortality" (142). (3) Sin "as the failure to recognize in all of one's fellow-beings ends rather than mere means;" and salvation from sin as "entrance into the joy of a life of love, of service, and of right relations with one's fellow-men, and of a consequent right attitude towards God" (144).

But in order to be effective a religion must be objectified in a person or persons. In a series of chapters on Religion and Family Life, Religion and Economic Life, Religion and Political Life, Religion and Social Pleasure, Jesus is presented as the adequate leader of this sufficient positive religion. His teaching is declared to be in agreement with the conclusions and demands of social science.

The distinctive feature of the book is the relation in which it places science—in particular the social sciences—to religion. Religion does not possess the knowledge it needs in order to exert a beneficial action upon society; it is absolutely dependent upon social science for guidance (218). In an important chapter, "The Essentials of a Social Religion," the author seeks to show that a world built in accordance with modern science would be the world of Jesus' vision, *i.e.*, a "Kingdom of God." "The goal towards which the best economic thinking in our civilization is moving" is a system "which will put human values first, which will emphasize economical obligations rather than economic rights, which will maximize coöperation both along private and along public lines, which will equalize opportunity, and finally which will make private wealth a trust held for public good" (241). These principles are "not different in essence from the principles laid down by Jesus in his dealings with economic questions" (241). The main postulates of the needed religion are "the supreme worth of men" and the attitude of service towards one's fellow men (162, 164).

But, one may object, if science formulates the principles which are to lead to the establishment of the ideal society (231), does not, then, the authority which has so far been exercised by religion and which is commonly supposed to belong to it, pass to science? Science and not Jesus becomes the leader. It is not clear to the reviewer that the author has realized the full significance of this conclusion, even

though the function he assigns to religion, in his more deliberate utterances, does not clash with that conclusion. The one specific task of religion is, according to him, to provide the energy, the devotion, the love, that will overcome the material and the human resistances to the establishment of the Kingdom of God on earth. Religion is "to release the creative energies of man" and "to help him to face the issues of life and death with confidence in himself and in his world" (161, 37). It must "furnish the driving motive if ever such (ideal) order is to be realized" (218).

The spirit in which this book is written as well as its substance should give it a wide and beneficent influence. It is the best book we know on the much discussed theme of the reconstruction of religion. We must, nevertheless, record here disagreement upon two points of fundamental importance to the argument.

There are, it seems to us, valid reasons against the adequacy of Jesus as the one and only leader in our present need. One is surprised and perplexed to find Ellwood ignoring as completely as he does Jesus' many limitations. Still greater astonishment may be felt at the unconcern with which he passes over the difference which he seems to admit between Jesus' conception of the divine Father and that which he describes as in agreement with modern science: "the conception of God which positive science arrives at is very different from many of the vagaries of speculative theology. It is more nearly in accord with the conception of God which Jesus held" (138). We are of the opinion that on this crucial point a conception of God only "more nearly" in accordance with science than inadmissible conceptions will not do. There remained too much of the Santa Claus and of the autocratic element in Jesus' conception of God to make it acceptable to modern science.

But even if these and other objections of the same category did not exist, the Prophet of Nazareth would seem to us unavailable as the leader of the needed positive religion. His personality and his teaching have been too much and too long misunderstood, and the misconceptions are too firmly embedded in age-old institutions not to have affected permanently his usefulness. The laws of social psychology which hold regarding other men's usefulness hold also with regard to that of Jesus.

The only other critical remark we wish to make here refers to the place assigned to religion or, we should perhaps say, to the meaning that is given to the term "religion." The original sources of

moral inspiration are not to be found in the religions. If we understand him correctly, Ellwood would not contradict that statement, and yet there is much in this volume that seems to imply a contradiction, as for instance, his belief that without a religion humanity would go to the dogs, and his affirmation that "true democracy was born from social religion" (250). Whatever power there is in the religions, is derived from the powers that are native to human nature; the religions are merely one of several categories or organized forms assumed by human needs and aspirations, in the presence of the universe, in order to their satisfaction or realization. Thus, the salvation of society is far from depending as solely upon the right religion, or any religion, as Ellwood would have us believe. There are in social life other stimuli of the generous, self-sacrificing, co-operative impulses than the religious ones. Devotion to noble causes does not require the theological beliefs of Ellwood's positive Christianity or of any other religion—unless one should use the term "religion," as some do, *i.e.*, as synonymous with whatever is generous and altruistic in human nature, in which case all useful discussion about "religion" would come to an end.

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MILLER, H. CRICHTON. *The New Psychology and the Teacher.* New York: Seltzer, 1922. Pp. 225.

VALENTINE, C. W. *Dreams and the Unconscious.* New York: The Macmillan Company, 1922. Pp. 140.

These books represent expansions of lecture courses given by their respective authors, and go to swell the rapidly increasing list of popular presentations of the "new psychology"—a rather lame term, but one used, like the "Great War," for lack of a better. Inasmuch as Dr. Miller is a physician writing primarily to teachers, and Dr. Valentine is professor of education in the University of Birmingham, the books have an interesting relation.

Most of the knowledge of the new psychology has been gained through actual clinical contacts, and this knowledge has been applied also to normal individuals. Obviously, Dr. Miller's work is sharpened with a considerable clinical experience. It would not be fair to accuse Dr. Valentine of a lack of such experience, but it seems unfortunate that he has dwelt so much, relatively, on second-hand clinical stuff rather than on the application of the new psychology to his own

particular field. Educators should have much to tell physicians in this connection.

Dreams and the Unconscious is described as an "Introduction to the Study of Psychoanalysis," and following the introduction there are chapters on Mental Conflict and Repression, Psychoanalysis through Dreams, Mental Analysis through Associations, Buried Complexes and the Discipline of Childhood, the Psychology of Dreams, and the Influence of the Unconscious in Everyday Life: (1) Sublimation and Sex, (2) Forgetting, Doubt and Prejudice. A bibliography and an index are appended.

Dr. Valentine's style is lucid, but there is present more than a suggestion of superficiality and of parlor application. Indeed, the task of describing even the framework of psychoanalysis in 140 pages almost compels a misleading simplification. He states in the preface that he has "particularly tried to bring the main doctrines into line with 'orthodox' psychology." The attempt is laudable at any rate. The author is skeptical of a universal application of wish-fulfillment to dreams (Freud himself has recently made an important modification of this explanation), and justly emphasizes the importance of "deliberate attempts at repression of unpleasant memories" in the causation of some dreams, and the understanding of others as "the completion of any mental process begun in waking life, but interrupted before coming to a satisfactory conclusion, or perhaps more particularly before being brought into unified relation with other impulses and thoughts, but without any further kind of repression; and the completion takes place through the energy, frustrated for the time, of that mental process itself."

Among Dr. Valentine's conclusions, we are told that "we can not, in the present state of knowledge, regard most dreams as of great significance for the understanding of normal individuals, useful as they undoubtedly are in the interpretation of some cases of nervous disease." And again, "in conclusion, we may admit that the main tenets of this new psychology of the unconscious have not yet been proved, so far as normal individuals are concerned. Absolute proof (or disproof) is, indeed, scarcely possible in such cases. But we are faced with a growing accumulation of evidence from several different sources that at least there is a large element of truth in some of its most important doctrines." The author evidently has some misgivings about his use of the term "normal," for he states in an explanatory note, "in fact, that 'normal' denotes not the majority but

only a minority." Which at least extends somewhat the range of applicability of the new psychology.

The New Psychology and the Teacher is "addressed not only to those who are professional teachers, but to the wider public of those whose business in life calls them to share in the teaching of the young." Dr. Miller utilizes the work of Freud and of Jung, he has an ear for the older psychology, and is not at all averse to indulging in some stimulating thinking of his own. There is, indeed, a "note of dogmatism" present, tempered somewhat by graceful gestures of humility. The ten chapters bear the following titles: Introductory; Authority and Suggestibility; Reality and Phantasy; Emotional Development: the Boy; Emotional Development: the Girl; the Unconscious Motive; Mental Mechanisms; Dream Symbolism; the Herd Instinct and the Herd Ideal; and Educational Methods.

In describing his conception of education Dr. Miller says: "There are roughly two aspects to education: the one, the transmission of racial experience; the other, the development of the individual psyche." These he identifies with "the presentation of authority and the presentation of reality." In the past, education has been too much of a ladling process, and inclined to be regardless of the depth of the receiving vessel. Of much greater importance is the development of the child's interests, which should tend to self-expression through actual accomplishment, but not a self-expression that is unmindful of social obligations. In this emphasis on the interests, the cultivation of attention which leads to self-control is not to be slighted, for otherwise, aside from the handicap which is associated with lack of attention, a train of neurotic symptoms may ensue. To the author, the goal of education is a self-realization which includes "the complete adjustment of the individual to life in all its aspects," and "the impulse towards growth is simply the primary biological urge to completeness which is found in every living thing." The central object of this growth is "ultimate parenthood, . . . the essential biological expression of maturity." He concludes his epitome of education so: "It is evident that, though this principal of growth is universal, it is not irresistible. It is infinitely liable to hindrance and deviation and delay at all points. The child's development towards completeness is very easily thwarted. If the urge to maturity is primarily biological, the barriers in its way seem to be almost invariably psychological; and for these barriers parents and teachers are commonly responsible. We put up a barrier when we restrain children unnecessarily; when we put difficulties in the way

of their self-expression; when without reason we demand that they should inhibit interest and activity which seem to them to be perfectly harmless. This is the barrier of authority. The second barrier is raised when we offer to the child a world that is too harsh, too puzzling and too difficult for its powers of adjustment. This is the barrier of reality. These are the two great problems for the child; and the test of his achievement is whether, when he reaches maturity, he has made the three great practical adjustments that life demands: the adjustment to society; the adjustment to the mate (actual or potential); and the adjustment to the Infinite. Failure at either of these points speaks of hindered development and the falling-short of complete self-realization." The discussion of suggestibility, phantasy, and emotional development is notable.

A proper note of warning is sounded to the effect that an appreciation of the new psychology is not for the purpose of analyzing children, but rather for the understanding it may give of the teacher's own psychology. This understanding to "increase his power to help the child in three principal ways—in his adjustment to reality, in his adjustment to authority and to the herd, and in his sex education." (The gender of the first pronoun is less applicable in the United States than in England.)

Dr. Miller takes some psychoanalysts to task for considering the individual *per se* rather than as a social unit, and deprecates the tendency to regard the herd instinct as a mere sexual manifestation rather than as the distinct entity claimed particularly by Trotter. He also directs a well-fashioned shaft at the rivalries between the psychoanalytic schools (by no means minimizing their differences), and prefaces this with the somewhat Utopian remark that "analytical views have spread so rapidly in the last eighteen years that the reactionaries will soon be negligible." While these rivalries have undoubtedly used up much energy and have afforded easier targets for the critics, it may be fairly doubted whether they will ultimately prove to be "obstructions" to the psychoanalytic movement as a whole. Internal dissension has ruined many good causes, but it may have constructive virtues also. In psychoanalysis it has stimulated inquiries and speculations along diverse lines, and it has brought about certain modifications. There is still much work for both probe and pruning-hook.

Dr. Miller's work is a refreshing application of the new psychology to a particular field—education—and goes to establish a relationship which should be of mutual benefit. His style has a

distinctive flavor, and even where he is not convincing he does not fail to hold the interest. *The New Psychology and the Teacher* deserves a large audience; it should be helpful to the teacher and parent, and to the physician it is a worth-while contribution to preventive mental medicine.

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